*Tolls.* By the Rev. J. S. Sidebotham, M.A. (Oxford, Shrimpton; London, Whittaker & Co.)—This is a short pamphlet by a clergyman who objects to pay tolls, partly from the natural dislike which we all entertain to that process, but chiefly, we think, because Nonconformist ministers do not pay and "the idea that *tolerated persons* are intended by law to have a privilege from which *legally authorised* persons are *the only class excluded*, is really too preposterous to be for an instant entertained on consideration by any person of ordinary intellect." The exemption of a clergyman on his parochial duty, whether the gate be within or without his parish, is plain enough. Whether a clergyman on temporary duty is exempt, and whether he may claim exemption when other persons are with him, are questions which have been often discussed, but never, we think, expressly decided. In the early part of the short pamphlet the author seems to assert that the case of Temple *v.* Dickinson decides both these questions in favour of the clergy, but before the end he admits that it does not, for the sufficient reason that the questions did not arise; but then the author is satisfied that if Lord Campbell were alive and the questions came before him he *would* decide them in favour of the clergy,—an argument that somewhat reminds one of the old song of Guy Faux. The questions are of some interest.

*Indian Year-Book for 1861. A Review of Social, Intellectual and Religious Progress in India and Ceylon.* Compiled by John Murdoch. (Nisbet & Co.)—Apologizing for the delay which has occurred in the publication of this summary of events which occurred so far back as 1861, Mr. Murdoch says, "some explanation is due for the late appearance of a Year-Book for 1861. The *primary* object of the work is to give facts about Missions. The Home Reports are not printed till about July, and another month elapses before they reach India. It is hoped, however, that the next issue will appear rather earlier, as well as be free from some of the imperfections which must be found in a first attempt." Amongst the imperfections of the present volume is the absence of a good general Index.

*The Diary of a Hunter from the Punjab to the Karakorum Mountains.* (Longman & Co.)—In the year preceding his death, at Meean Meer, where he fell in the August of 1861 whilst bravely ministering to his sick soldiers of the 51st King's Own Light Infantry, Lieut.-Col. Henry Augustus Irby made a hunting excursion from the Punjab to the Karakorum Mountains. The journal kept by the Colonel during this six months' trip is now offered to the public by his brother. Indian sportsmen will turn over its pages with pleasure, and the writer's personal associates will accept it as an agreeable memorial of an officer whose manly and devout nature won the affections of all who knew him.

*The Trial of the Constitution.* By Sidney George Fisher. (Low & Co.)—"The flight of events," observes Mr. Fisher, "is now so rapid that he who wishes to influence opinion must speak quickly, and cannot therefore bestow much time on careful and artistic execution." The writer of these words does not now need to be informed that his volume appears too late to be of any service to the North. Indeed, ere he took pen in hand the time had passed when his views might have produced a transient effect.

Of Miscellaneous publications we have to mention:—*A Comparison between Iron-Clad Ships with Broadside Ports, and Ships with Revolving Shields*, by Capt. Coles (Stanford),—*Our New Mode of Cupola Ship—H.M.S. Enterprise*, by Rear-Admiral Halsted (Nichols),—*Military Despotism; or, the Inniskilling Dragoon, a Tale of Indian Life; To the Members of the House of Commons* (Chapman & Hall),—*Garrotting; or, is the Ticket-of-Leave System a Failure?* by One who Watches (Hotten),—Mr. Symons *On the Distribution of Rain over the British Isles* (Stanford),—*The Maze of Banking*, by a Depositor (Simpkin),—*The Colony of Rupert's Land: Where is It, and by what Title held?* by Capt. Synge (Stanford),—*Ireland's Right and Need: Self-Government, a Letter to the Earl of Carlisle*, by W. Smith (Kelly),—*An Exposure of the Extraordinary Persecution of Dr. Domingo Gounouilhon, by the Authorities of Montevideo* (O'Byrne),—*The American War Crusade; or, Plain Facts for Earnest Men*, by J. R. Balme (Hamilton),—*Honesty is the Best Policy, an Apophthegm submitted (without permission) for the Consideration of the Right Hon. Sir Charles Wood*, by a Late Company's Officer (Wilson),—*Notes on the Rate of Mortality in Manchester*, by Dr. Whitehead (Ireland),—*A Report upon some of the Colonial Medicinal Contributions to the International Exhibition*, by C. Hunter (Churchill),—*The Plain English of American Affairs*, by J. Worden (Bennett),—*A Reply to the Address of the Right Hon. the Earl of Shaftesbury and others to the Right. Hon. Sir C. Wood on the Proposed Law of Contract for India*, by W. F. Fergusson (Ridgway), and *The System of Landed Credit; or, La Banque de Crédit Foncier, the Working of that Institution in Europe, the Introduction of the System into Lower Canada briefly considered*, by G. H. Macaulay (Quebec, Desbarats & Derbishire).

LIST OF NEW BOOKS.

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Bessy's Money, by the Author of 'Mary Powell,' roy. 18mo. 1/ cl.
Bibliotheca Classica, ed. by Long, 'Vergil, by Conington, Vol. 2,' 14/
Black's Guide to London and its Environs, 2nd edit. fc. 8vo. 3/6 cl.
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## THE DOCTRINE OF HETEROGENY AND MODIFICATION OF SPECIES.

Down, Bromley, Kent, April 18.

I hope that you will permit me to add a few remarks on Heterogeny, as the old doctrine of spontaneous generation is now called, to those given by Dr. Carpenter, who, however, is probably better fitted to discuss the question than any other man in England. Your reviewer believes that certain lowly organized animals have been generated spontaneously—that is, without pre-existing parents—during each geological period in slimy ooze. A mass of mud with matter decaying and undergoing complex chemical changes is a fine hiding-place for obscurity of ideas. But let us face the problem boldly. He who believes that organic beings have been produced during each geological period from dead matter must believe that the first being thus arose. There must have been a time when inorganic elements alone existed on our planet: let any assumptions be made, such as that the reeking atmosphere was charged with carbonic acid, nitrogenized compounds, phosphorus, &c. Now is there a fact, or a shadow of a fact, supporting the belief that these elements, without the presence of any organic compounds, and acted on only by known forces, could produce a living creature? At present it is to us a result absolutely inconceivable. Your reviewer sneers with justice at my use of the "Pentateuchal terms," "of one primordial form into which life was first breathed": in a purely scientific work I ought perhaps not to have used such terms; but they well serve to confess that our ignorance is as profound on the origin of life as on the origin of force or matter. Your reviewer thinks that the weakness of my theory is demonstrated because existing Foraminifera are identical with those which lived at a very remote epoch. Most naturalists look at this fact as the simple result of descent by ordinary reproduction; in no way different, as Dr. Carpenter remarks, except in the line of descent being longer, from that of the many shells common to the middle Tertiary and existing periods.

The view given by me on the origin or derivation of species, whatever its weaknesses may be, connects (as has been candidly admitted by some of its opponents, such as Pictet, Bronn, &c.) by an intelligible thread of reasoning a multitude of facts: such as the formation of domestic races by man's selection,—the classification and affinities of all organic beings,—the innumerable gradations in structure and instincts,—the similarity of pattern in the hand, wing or paddle of animals of the same great class,—the existence of organs become rudimentary by disuse,—the similarity of an embryonic reptile, bird and mammal, with the retention of traces of an apparatus fitted for aquatic respiration; the retention in the young calf of incisor teeth in the upper jaw, &c.,—the distribution of animals and plants, and their mutual affinities within the same region,—their general geological succession, and the close relationship of the fossils in closely consecutive formations and within the same country; extinct marsupials having preceded living marsupials in Australia, and armadillo-like animals having preceded and generated armadilloes in South America,—and many other phenomena, such as the gradual extinction of old forms and their gradual replacement by new forms better fitted for their new conditions in the struggle for life. When the advocate of Heterogeny can thus connect large classes of facts, and not until then, he will have respectful and patient listeners.

Dr. Carpenter seems to think that the fact of Foraminifera not having advanced in organization from an extremely remote epoch to the present day is a strong objection to the views maintained by me. But this objection is grounded on the belief—the prevalence of which seems due to the well-known doctrine of Lamarck—that there is some necessary law of advancement, against which view I have often protested. Animals may even become degraded, if their simplified structure remains well fitted for their habits of life, as we see in certain parasitic crustaceans. I have attempted to show ('Origin,' 3rd edit. p. 135) that lowly-organized animals are best fitted for humble places in the economy of nature; that an infusorial animalcule or an intestinal worm, for instance, would not be benefited by acquiring a highly complex structure. Therefore, it does not seem to me an objection of any force that certain groups of animals, such as the Foraminifera, have not advanced in organization. Why certain whole classes, or certain numbers of a class, have advanced and others have not, we cannot even conjecture. But as we do not know under what forms or how life originated in this world, it would be rash to assert that even such lowly endowed animals as the Foraminifera, with their beautiful shells as figured by Dr. Carpenter, have not in any degree advanced in organization. So little do we know of the conditions of life all around

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us, that we cannot say why one native weed or insect swarms in numbers, and another closely allied weed or insect is rare. Is it then possible that we should understand why one group of beings has risen in the scale of life during the long lapse of time, and another group has remained stationary? Sir C. Lyell, who has given so excellent a discussion on species in his great work on the 'Antiquity of Man,' has advanced a somewhat analogous objection, namely, that the mammals, such as seals or bats, which alone have been enabled to reach oceanic islands, have not been developed into various terrestrial forms, fitted to fill the unoccupied places in their new island-homes; but Sir Charles has partly answered his own objection. Certainly I never anticipated that I should have had to encounter objections on the score that organic beings have not undergone a greater amount of change than that stamped in plain letters on almost every line of their structure. I cannot here resist expressing my satisfaction that Sir Charles Lyell, to whom I have for so many years looked up as my master in geology, has said (2nd edit. p. 469):—"Yet we ought by no means to undervalue the importance of the step which will have been made, should it hereafter become the generally received opinion of men of science (as I fully expect it will) that the past changes of the organic world have been brought about by the subordinate agency of such causes as Variation and Natural Selection." The whole subject of the gradual modification of species is only now opening out. There surely is a grand future for Natural History. Even the vital force may hereafter come within the grasp of modern science, its correlations with other forces have already been ably indicated by Dr. Carpenter in the *Philosophical Transactions;* but the nature of life will not be seized on by assuming that Foraminifera are periodically generated from slime or ooze.

Charles Darwin.

## THE ANTIQUITY OF MAN.

10, Kent Terrace, April 20, 1863.

It is with regret that I find myself at issue with the Author of 'The Antiquity of Man.' I could have wished to have avoided any controversy on the subject, as I hope at some future period to have a fitting time and occasion for my own account of the inquiry; but there are portions of Sir Charles Lyell's letter to the *Athenæum*, of the 18th inst., in reply to Dr. Falconer's letter in the number of the 4th inst., which call for some brief notice on my part. I would most willingly have commented on the proofs of Sir Charles Lyell's important work had they been submitted to me before the publication of the first edition: not having had that opportunity, I found myself obliged to report to Sir Charles, when he wrote to ask me for a list of errata and corrections for the second edition, that "I raised objection to the tone and cast of some chapters, and that the corrections I might think necessary would involve more alteration than was practicable or than could originate with me." I referred as an example to the Bedford case, "on which so many important geological questions hinge." Sir Charles in his reply informed me that, after referring to the published accounts of it, he did not see what he had to alter. It is possible that I may not have been sufficiently explicit. I should regret if it were so.

With regard to that particular case, I can only repeat the statement that I made to my friend, Dr. Falconer, that the Bedford section was made out by me long before the period of Sir Charles's visit there; that the main features were pointed out by me to him on that occasion; and that I further brought a short notice of Mr. Wyatt's interesting discovery together with the first geological description of the section before the Geological Society, in March, 1861,—somewhat prematurely, possibly, for being part of a general inquiry, in which, as Sir Charles knew, I had been engaged for some years, I should have waited until I could have brought forward the whole subject (long unavoidably delayed by the limited measure of time I can take from active business avocations), but for its special bearing on the question of the Antiquity of Man, and the publicity given to this case. Only those engaged in the study of the quaternary deposits, and who know how difficult it is to obtain definite facts, and how many days and years may be spent in examining ground which affords only negative evidence, can understand the importance of a good positive case like that of Bedford. I quite agree with Sir Charles Lyell in his observations about too frequent references to original authorities in a popular work: it may even be a question whether the general reader may not consider such references to authorities and to companionship already too frequent in 'The Antiquity of Man.' No doubt, as Sir Charles observes, the public generally are satisfied to learn from him his own conclusions in as few words as possible; but he must remember that he is also addressing a large scientific public, and that it is not a question of frequent but of accurate reference that is contended for. I am satisfied that whatever may have been the intention of Sir Charles, his readers must form a very inaccurate idea of the important part taken for many years past by Dr. Falconer in researches connected with the antiquity of man, in the investigation of bone caves in general, and of the Brixham cave in particular, as well as of the relative part taken by the various geologists named by Sir Charles, and by Sir Charles himself, in other parts of the investigation. I have been greatly interested in the progress of the Brixham cave exploration, and can fully corroborate Dr. Falconer's account of it; and this misapprehension is another reason which makes me regret the delay in the publication of the final results.

Sir Charles Lyell is perfectly correct in saying that I have modified my views since the publication of my first memoir (not memoirs) on this subject. But I would remark that that paper was read before the Royal Society in the month following my first visit to the Somme Valley and to Hoxne, and that in it I contented myself with a description of the ground and with the determination of the geological age of the deposits—points which remain unimpugned—and stated that I reserved my views on the theoretical questions for further inquiry and research. After an interval of three years, I brought these forward in a memoir, read before the Royal Society in March, 1862; and although my views had, I admit, been modified and matured, the main question of the post-glacial age of the beds was confirmed by various new sections; whilst, although feeling that the period concerned is one of very remote antiquity, I still adhere to the opinion I had before expressed, that the evidence does not carry man back in past times more than it brings forward the great extinct mammalia towards recent times.

One of the great charms of scientific inquiry lies in the free and intimate intercourse and interchange of ideas amongst men engaged in the same branches of research. In such intercourse, where each observer contributes his facts or his opinions, the starting-point of some of these must often be lost to view, and all men of science must, at times, have felt and experienced that, in the lapse of time, an unconscious process of greater or lesser mental assimilation unavoidably takes place. It is, therefore, only when certain limits are passed, albeit inadvertently, that any one would care or think fit to object.

Every geologist must feel indebted to Sir Charles Lyell for the philosophical spirit he has brought to bear in geological inquiry, and all must admire the untiring energy with which he has for years past investigated the phenomena he describes. Having studied with him in the field many of the complicated phenomena of the post-pliocene deposits, while I claim as my share of the work the detection and the interpretation of certain physical phenomena, I am free to acknowledge the pleasure and advantage I have received from the discussion of the various questions arising therefrom with a geologist so experienced and philosophical as Sir Charles Lyell.

Joseph Prestwich.

## THE NEW ZEALAND MOAS.

April 22, 1863.

A paragraph is now going round the papers stating that, just before the mail left, one of the most gigantic of birds, a Moa or Dinornis, and believed to be extinct, had been seen alive in New Zealand, and that an enterprising colonist had offered a reward of 500*l.* for its capture, dead or alive. The public seem to be divided respecting the amount of credence to be attached to the story; but the fact that a gentleman residing on the spot thought it worth while to offer a handsome reward would seem to show that there was, in his judgment, some probability on the very face of it. That some of the smaller species of Dinornis may still be alive is an opinion which even Prof. Owen, if I understood him rightly, entertains. If extinct, the Moas have become so probably in quite recent times—that is to say, *since* the occupation of New Zealand by the Maoris. This opinion, I think, may be supported by philological arguments, briefly stated in my Official Reports on the Fiji Islands, presented to Parliament, May, 1862, and also in my 'Viti,' p. 383, where I said:—"'Toa' is the Fijian form of the word 'Moa,' applied throughout Polynesia to domestic fowls, and by the Maoris to the most gigantic extinct birds (*Dinornis*, sp. plur.) disentombed in New Zealand. The Polynesian term for birds that fly about freely in the air is Manu or Manumanu; and the fact that the New Zealanders did not choose one of these, but the one implying domesticity and want of free locomotion in the air, would seem a proof that the New Zealand Moas were actually seen alive by the Maoris about their premises, as stated in their traditions, and have only become extinct in comparatively recent times."

Berthold Seemann.

## SCIENTIFIC BALLOON ASCENT.

Blackheath, April 21.

In the *Athenæum* of the 11th inst. are detailed the observations I made on the sky spectra in the Balloon Ascent on March 31. They were so different from what I expected that I could not avoid coming to the conclusion, that they were of little value in consequence of the ascent having been made so late in the day. I therefore resolved that the next ascent should be made when the sun was near the meridian, and that the spectrum examination should be a primary subject of investigation. The apparatus was the same as that used on the previous experiments. It was covered with black cloth to prevent any stray light falling on the prism, and whilst observing my head was also covered with black cloth. Between the hours of 11 A.M. and noon, I examined the solar and sky spectra with care. The sky was generally covered with cumuli, and there was a great mist. The solar spectrum extended from B to H nearly; and the sky spectrum from B to G, but these were quite its limiting lines.

We left the earth on April 18 at 1h. 17m. P.M.; within two minutes afterwards we were 3,000 feet, and at 1h. 23m. we were one mile high. The second mile was passed at 1h. 29m.; the third at 1h. 37m.; the fourth at 2h.; and the highest point was reached at 2h. 30m.—at the height of four and a half miles nearly. At 2h. 36m. we passed below four miles; the next mile downwards was passed at 2h. 40m.; and at 2h. 46m. we were two miles from the earth, which we reached at 2h. 50m. At 1h. 20m. looking close to the sun, the line G was very clear, as well as the two nebulous lines H, and the spectrum extended somewhat further; many lines were seen. At 1h. 21m. at the red end of the sky spectrum near the sun, the line B was very clear, and many lines between B and F were visible. At 1h. 28m. the sky spectrum under and close to the sun extended from A at the red end to beyond H, the lines were beautifully defined, and I thought somewhat more numerous than as viewed from the earth. At 1h. 23½m. the sky spectrum at some little distance from the sun did not reach to G, and scarcely to B; but there were many lines between these extremes. At 1h. 33m. on directing the slit to the sky far from the sun, the field of view was dark. At 1h. 37m. as the balloon was revolving I had a beam of light from the sun, whilst looking at the red end, and all lines were clear up to A. At 1h. 39m. the slit was directed to a point in the sky as near the zenith as the balloon permitted, and the spectrum was

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